STANFORD UNIVERSITY MEDICAL CENTER

DEPARTMENT OF GENETICS

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Dr. George W. Beadle
5533 Dorchester Avenue
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Dear Beets:

I can certainly sympathize with your situation vis-a-vis the memoir of Ed Tatum, and I would have responded with more initiative perhaps somewhat sooner, but wanted to be quite clear that you had had an untrammeled opportunity to take your own steps in the matter.

I am still in the midst of a rather more detailed biography and scientific history -- which is obviously what my questions about the origins of Neurospora were all about -- which I will eventually send to the National Academy of Sciences. When it is ready, I will of course send you a draft for your more detailed reactions and comments.

I have gone far enough, however, that it was a fairly small effort to produce the rather brief version which I am now enclosing. Needless to say, I would be very happy to persevere in our agreed arrangement of co-authorship, and in particular if there are any changes or additions that you would wish to suggest. Alternatively, if you prefer that I just put it in under my own name substantially as it now stands, that would also be understandable. It is rather difficult to speak accurately and appropriately about people and work with which one has been so deeply involved personally, and I can just hope that the obvious sources of confusion of judgment are not too serious for my own part in what I am sending you. Anyhow, please let me know your pleasure and I will gladly follow accordingly.

I was in New York last week and was lucky enough to be able to discover Ed's original Neurospora notebooks in the Rockefeller archives. I believe Ray Barratt had inherited them and had seen that they were then preserved. Fortunately, they do now clarify the detailed history down to months and dates and you might be interested to see the page or two that I have copied out for you. It is clear that the nutrition of Neuropsora was worked out in a matter of a few days, the biotin requirement having been established on March 18, 1941. I have also gotten Carlton Schwartz's course notes for the course in Comparative Biochemistry which match these dates rather nicely. The notebooks have a gap in dates between March and July 14th when number 299 is mentioned for the first time. I would assume that the work on irradiation and strain isolation was going on in your own laboratory and would have been reflected in a different set of notebooks. Again, it seems to have been a matter of only a few days to pin down the B_6 requirement of this strain, establish quantitative growth relationships and so forth. I would also

assume that the genetics of the mutant was being pursued in parallel in your own shop. The first paper describing these results was communicated to the PNAS in October and appeared in the following month.

So I think there is now a good documentary record that the work was begun in the middle or towards the end of February, and yes indeed, it was an extraordinarily rapid development for such a pivotal accomplishment.

It has been a bit of detective work to unearth these different sources, but I am glad to see that they are finally converging into a verifiable story despite the understandable confusions of retrospective recall over such a long period of time.

Yours sincerely,

Joshua Lederberg
Professor of Genetics

Enclosure

JL:ek-f